

## OPEN LETTERS.

### The nomenclature question: Concerning homonyms.

In the September number of the GAZETTE Mr. Coville remarks that "probably the greatest objections that can be urged against the Association principles of nomenclature are those which may be brought forward relative to this very rejection of homonyms." But his explanation and examples do not in any way lessen the force of the criticisms of Dr. Robinson in the preceding number. This seems to be because no one, so far as I am aware, has yet properly distinguished between the rejection of revertible names within the limits of a genus, and their rejection when due to the varying conception of the limits of a genus. And this leads me to propound the question, What is a homonym? Among genera a name applied to one genus is never thereafter applicable to any other, and if so applied it becomes a homonym. Why should it not be equally true that a binomial which has been applied to one species is never thereafter, under any circumstances, applicable to any other species? In disregard of this principle, the recently published Check-List adopts the name *Anychia Canadensis* B. S. P. (1888), whereas it is acknowledged on the same page that Elliott applied this identical name to another plant as early as 1817! And thus a homonym, in the strict sense of that word, is adopted merely on the ground that the specific name *Canadensis* is older as applied to the former species! I do not understand by what stretch of the imagination *Anychia Canadensis* Ell. can be considered a homonym of *Queria Canadensis* L. In the same Check-List *Lespedeza frutescens* is proposed as a new name, notwithstanding the fact that Elliott, in 1824, used the same combination for a different plant. This, it appears to me, is a wholly indefensible and extremely pernicious principle.

Let us take another case. Every one will acknowledge that the two genera *Silene* and *Lychnis* are very closely related, and that some botanist might at any time unite them. If this were done, *Lychnis alba* Mill. (1768) would, according to the Check-List principles, become *Silene alba*, and *Silene alba* Muhl. (1813) would be relegated to the synonymy—there to remain forever—*Silene nivea* being substituted for it. Now suppose that the next writer upon this group should consider the two genera distinct. Again we would have *Lychnis alba* Mill., and the nomenclature of the two genera would stand as it does in the Check-List to-day, save that *Silene alba* Muhl. (a name which is now unchallenged) would have become *Silene nivea*, and must forever remain so, unless by some "lumping" or other jugglery this name too should be rejected, when a new one would have to be concocted. To generalize: if, of two large and closely related genera, A and B, A has the older name, the mere act of temporarily uniting B with A and then separating it again, may change permanently the names of some species of A, although these names may be otherwise unobjectionable. Can a rule which will permit this condition of things be a sound one?



Both Dr. Robinson and Mr. Coville express their dislike for theoretical objections. But what other objections can be made to a theoretical rule? It must be remembered that the principle under discussion has never been put into practice by any botanist who is inclined to "lump" related genera. Such botanists have existed in the past, and are sure to arise in the near future, and should they ever write in accordance with this principle, the present theoretical objections will at once become actual and overwhelming ones, and the present supporters of this principle will wake up, rub their eyes, and wonder why their rules don't work. It is my claim that nomenclatural rules, to be permanent, should provide for all foreseen possibilities.

It may be claimed by some that the utter disregard of the right of priority of a binomial, as such, is a principle copied from the zoologists. If so (and I am not sufficiently acquainted with their usages either to deny or to affirm it) I feel constrained, as does Dr. Millspaugh in considering decapitalization, "to speak against the tendency of many botanists to follow a bad example set by zoologists."<sup>1</sup>

To sum up: it appears to me that the recognition of the *priority of binomials*, and, so long as it does not conflict with the former, of the *priority of specific names*, possesses all of the advantages, and none of the disadvantages, of the *rejection of revertible names*. It will thus be seen that I am a firm believer in priority law, only consistency requires me to recognize the right of priority for binomials; and further, I believe heartily in the rejection of homonyms, provided only the word "homonym" be taken in its proper sense. The suggestions here brought forward would settle the case of *Juncus megacephalus* and all other cases which Mr. Coville has presented, in the way in which he would like to see them settled, and at the same time would do away with Dr. Robinson's objections, as published in the August GAZETTE. I feel sure that the leaders of reform, with whom I am thoroughly in sympathy, will strengthen their cause by rejecting any weak principle, instead of clinging to it when it has been proven untenable.—JOHN HENDLEY BARNHART, Tarrytown, N. Y.

#### Decapitalization.

Opposed, as I am, to the spelling of names derived from persons without a capital letter, I should nevertheless like to ask Mr. C. F. Millspaugh a question or two. How can *nevadensis* ever mean *of the whiteness of snow*? Is there a Latin scholar in existence who would not understand that *bajaensis* means *coming from Baja*? Is it not the case that in nearly all languages, except English, the adjectival form of place names is written with a lower case initial, while only the substantival form is written with a capital? In short, should we not write *Scutella blaviensis*, but *Scutella Besanconi*, *Scutella calvimontana*, but *Scutella Morgani*? Mr. Millspaugh will observe that I write this entirely from the point of view of the classical scholar, and not from the point of view of the systematic biologist, who must, I fear, be left to his ineptitudes whether he be zoologist or botanist.—F. A. BATHER, British Museum.

<sup>1</sup> Bot. Gaz. 20: 429. 1895.